Semiotix Course 2008, The epistemology of Pleistocene archaeology

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Lecture No. 7. **A metamorphology of archaeology**

Science expects exacting predictions for future observations about phenomena that can be measured. The regularities within these phenomena must be described as consistent patterns, explained by refutable theories cast in terms of causes. Metamorphology, as the science of how the perception of the individual archaeologist about what happened in the past relates to what *really* happened in the past, needs to analyze the epistemology of how archaeological data are collected, interpreted and disseminated. As a theoretical framework it is essentially predicated on the application of integral functions to all unknowns in archaeology. Metamorphology thus creates systematic uncertainties, but has the enormous benefit of being falsifiable, and of offering us unprecedented opportunities to test conventional, and otherwise untestable archaeological propositions. It needs to peruse each instance of inductive uniformitarianism and project it onto the canvas of the individual researcher's cognitive, perceptual, religious, political, ontological, academic and intellectual conditioning. It particularly needs to understand his limitations of knowledge concerning existing data, consequences of his language barriers and personal or cognitive biases. All of these factors act as very effective filters in how evidence may be perceived, interpreted and reported. The variables of the individual researcher's limitations should certainly not be immune to investigation; they are legitimate targets of research (see Lectures 2 and 6). And the number of possible systematic biases is incredible: confirmation bias, *déformation professionnelle*, selective perception, reactance, neglect of probability, wishful thinking bias, Von Restorff effect, outcome bias, framing effect, bandwagon effect, expectation bias, congruence bias, attentional bias, clustering illusion (apophenia), conjunction fallacy, Hawthorne effect, observer-expectancy effect, primacy and recency effects, and most especially, selection bias.

The collection of archaeological data

In the previous lecture we have seen that the basis of a sound epistemology of Pleistocene archaeology is the application of metamorphology. As its name indicates, this is the science of changes in the forms as which evidence seems to present itself, changes from the intrinsic to the subjectively observed. We can divide these changes broadly into physical and philosophically based types. The physical changes are those attributable to taphonomy—including decay, trampling, transport, mineralization and many other processes affecting all kinds of material remains forming what is simplistically called the 'archaeological record'. The cognitive aspects of metamorphology concern cognizance of the way archaeological percepts are formed independent of sense-data (sensu Russell 1959: Ch. 1), as freestanding constructs of individual perceptions. Post-processualist archaeology, we noted in Lecture 3, fully accepts that all data are theory laden, and in the case of this discipline, their qualitative range and their quantifications are all determined arbitrarily, i.e. there is no falsifiable taxonomy to guide us. Thus the raw data of Pleistocene archaeology are collected in accordance with classification systems whose ultimate veracity is not accessible to external scrutiny. That does not render them necessarily false, but in using them it is essential that this be clearly appreciated. The artifact types we recognize, tabulate and record are generally invented constructs ('archaeofacts'), as are many other entities recorded as supposedly empirical phenomena, ranging from perceived relations to landscape to interpretations of intent.

Even the apparently 'more scientific' aspects of such practices can and need to be questioned. For instance one of archaeology's most important practices is the production of perceived sediment stratigraphies from excavated profiles. Without it, there can be little archaeological discourse. And yet, this could be demonstrated to be a very subjective procedure. Suppose one were to conduct a 'blind test', locate a complex stratigraphic section and ask, say, ten archaeologists to independently draw the same deposit's stratigraphy. Almost certainly this would yield ten different drawings, 'ten different stratigraphies'. The reasons for this are diverse: proper sedimentary analyses are not conducted during most excavations; samples might be taken but are usually processed much later in a laboratory. Most archaeologists have inadequate understanding of sediments, and therefore these section drawings are generally the result of simple eyeballing. They are essentially individual artistic and unique works, which means that their derivation is not a

Figure 1. Typical complex rock art panel, a favorite subject of archaeologists in inventing styles, traditions and cultures. Al-'Usayla, Saudi Arabia.

repeatable process, i.e. it is untestable. In practice, only one such section drawing is usually secured of a particular stratigraphy, and then the section itself is most often destroyed by continuing excavation or by the elements. Therefore no procedure of refutation is feasible; we have to take the section drawing purely on trust; it contains the only surviving evidence of the former stratigraphy.

It is not suggested that this practice should be revised; rather, it just needs to be illuminated here what has to be understood in the context of metamorphology. We need to appreciate that the section drawings of Pleistocene sediments, although often of extraordinarily high analytical quality, are not necessarily statements of 'facts', but are interpretations whose veracity depends solely on the competence of the recorder. The kinds of sediments archaeologists tend to excavate are complex deposits that are not readily understood, and yet the veracity of archaeological claims hinges largely on their correct interpretation. Just as our taxonomies of artifacts derive from 'egofacts' (Consens 2006), a variety of further dimensions the Pleistocene archaeologist may perceive as facts are also self-confirming phenomenon categories.

This is readily illustrated with rock art, which is traditionally approached from two but complementary directions by archaeologists: they either seek to determine its meaning (especially what it depicts) or invent a taxonomy of its motifs, often to use as the basis of creating purported sequences of traditions. Although both these approaches lack epistemological justifications, they constitute essentially what is generally being considered to be an 'archaeology of rock art' (Chippindale 2001; Chippindale and Taçon 1998). Archaeologists are hardly in a better position than other cultural aliens to determine what is depicted in rock art. They derive these interpretations by the same means as everyone else: through autosuggestion, the psychological process by which the individual induces self-acceptance of an opinion or belief. In the absence of any sound ethnographic input, taxonomic entities or systems of rock art are autogenous, etic constructs of untutored observers, irrespective of the training of such people.

The subjective constructs derived from invented taxonomies of rock art are routinely used as a basis of considerations of style, and in attempts of defining cultural traditions and their sequences (Fig. 1). This approach is the basis of archaeological dating of rock art, by then relating these etic definitions to cultures and their sequences that have been perceived from excavating occupation layers. The epistemology of this approach contains numerous logical faults. To begin with, the 'cultures' the Pleistocene archaeologist names and defines are not reflections of reality, they are inventions, based on modern opinions of self-appointed experts of the human past about what constitutes cultural traditions of these ancient times. There is no testable or refutable evidence that these 'cultures' marked any ethnic, political, social or linguistic groups, tribes or nations. They are identified largely on the basis of minor differences in the stone tools they used. Obviously tools do not define cultures, their characteristics merely reflect ideas and practical conventions. Such ideas or 'memes' may travel or they may be re-invented independently elsewhere. Here we meet again this fundamental trait of orthodox Pleistocene archaeology: the notion that movement of qualitative indices of material finds proves movement of people, and here it is stone tools that are the fetishes representing people. Art or symbolic behavior, in contrast, is far more culture specific, but the discipline has historically chosen to base its taxonomy on stone tools, i.e. on technology rather than culture.

So in Ice Age archaeology, cultures have not been defined correctly, because instead of determining cultural taxonomies we have established what appear to be technological taxonomies. To then force cultural variables such as palaeoart into these non-cultural pigeonholes is absurd. It would have been more logical to try establishing cultural parameters and sequences, and to then fit the technologies into this framework. But even this is usually not possible, and it is again illustrated by the ways archaeology approaches rock art—which are in fact amazingly 'un-archaeological'. Typically, major sites comprise complex superimposition sequences spanning many cultures, because widely separated societies contributed to the same rock panel. Some of the artists may even have reacted to pre-existing art that no longer survives. Yet the archaeologist, using a low-resolution approach to the 'study' of this cumulative corpus, tends to treat it as a single entity, as a representative sample. Unable to determine the ages of these chronological components, the archaeologist might invent styles to separate them. But where physical superimposition of motifs is largely lacking, as is most often the case, these 'stylistic constructs' are again merely freestanding formulations generated by autosuggestion. So in contrast to an excavation, which of-

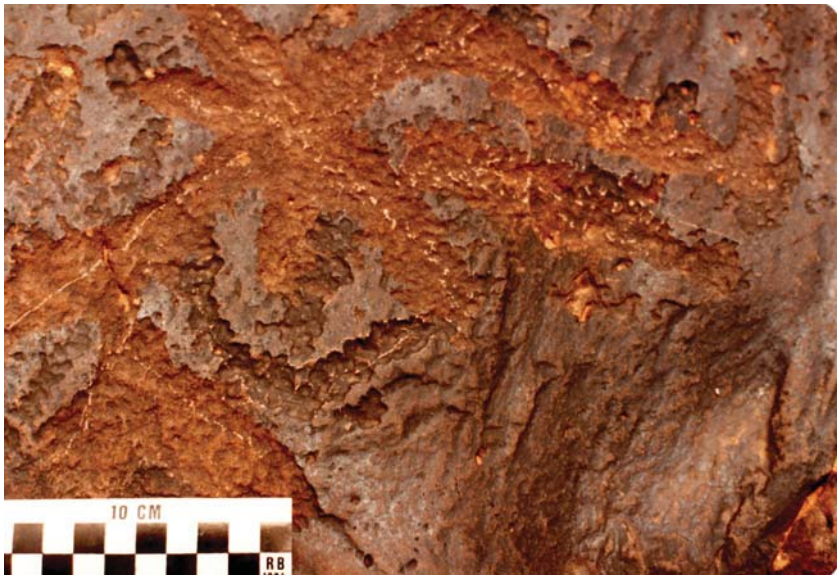

Figure 2. Invented style of Australian petroglyphs, the 'Panaramitee style', at the type site Panaramitee North. It comprises elements of different periods arbitrarily lumped together; note the vastly differing ages of the petroglyphs.

fers a layering of successive traditions in three-dimensional space, the successive traditions of a major rock art site occur in two-dimensional space; they have no archaeological time depth. In many cases archaeologists then assume that only one tradition has contributed, and they call this a style. There are numerous examples of such 'styles' that are in fact the precipitates of several traditions (e.g. the 'Panaramitee style' in Australia) (Fig. 2). This practice of lumping together all components of a site is logically identical to excavating layers of the Paleolithic, Neolithic and Bronze Age and then lumping together all artifacts to define them as belonging to a single culture. No archaeologist would do this with excavated remains (at least not deliberately), but with rock art it is a frequent practice, unless there is some really blatant contrary evidence available.

The example of rock art to illustrate how an epistemological analysis shows that the fundamental assumptions about sites may be completely wrong can be extrapolated to many other practices or areas of Pleistocene archaeology. Indeed, there are many scenarios in archaeology where the correct interpretation of a variable may be the exact opposite of what the researcher might be inclined to think. For instance there is the tendency, already touched upon in the context of rock art, to make implicit assumptions about different evidence types being related because they occur at the same site. Most sites excavated by Pleistocene archaeologists are 'favored localities' in the landscape, for obvious reasons: it is where one tends to find things. They may be caves, rockshelters, desert springs and so forth. Many such caves, for instance, have been lived in by tens of thousands of individuals (over hundreds of millennia). Therefore the probability that two unconnected forms of evidence (e.g. rock art and a hearth) date from the same visit is infinitesimally small. Conversely, the probability of any two forms of anthropic evidence being of the same visit is millions of times greater at a featureless site in a huge featureless plain (Bednarik 1989).

Clearly, then, there are systematic and hence assessable cognitive biases in the way data are collected in the field—biases that relate to the collecting researcher: to her/his methodology, funding limitations, time constraints, limitations of knowledge and expertise, and other such variables. As any practitioner will readily admit, the quality of archaeological fieldwork varies enormously, and such factors are to some degree measurable. But how does one quantify factors such as individual knowledge with any semblance of objectivity? It is obviously possible, with some diligent research, to form subjective views of a researcher's competence, but this would at best provide a very coarse tool of analysis, and be misleading at worst. It is the task of metamorphology to develop such principles in testable, repeatable and transparent formats.

A review of archaeological interpretation

Unless we can understand how data were collected there is no metamorphological depth in the presentation of observations. But not only are data *collected*, they are also *stored*, *interpreted* and *disseminated*. Each of these steps is subject to a whole range of biases. For instance, archaeologists may assume that charcoal contained in a sediment layer indicates the age of the layer, relying on an associative hypothesis. In an epistemologically sound system all components of a sediment layer are of different 'ages', and there is no guarantee that charcoal can date the event of the sediment's final deposition. Numerous common processes can distort the relationship. Secondly, most archaeologists mistakenly believe that charcoal is the age indicated by its content of carbon isotope ^{14}C. This is false for several reasons, among them the fact that this index does not even relate to the event of carbonization of the wood, but to a process of assimilation in a live tree. Or in epistemological language, we have a supervenience condition between two propositions, both of which are factually false. Yet archaeologists habitually speak of the 'radiocarbon age of a sediment', which is a nonsensical concept.

Or to use a differently framed example to illustrate the endemic issue, consider the idea of micro-wear analysis. It may be very useful and seems to exhibit the trappings of a scientific method, but it is *not* scientific; it does not satisfy the requirement for falsification. The observation that microscopic wear on an archaeological specimen resembles experimental wear on another does not prove that two similar processes pertain. Again, the dependency relation is one of supervenience: there could be a difference in one set of properties without there being one in the second. In other words, *modus ponens* is not valid. The proposition that the traces on the ancient specimen were made by the same process as those on the replication artifact is not refutable. It may well be true, it quite probably *is true*, but

Figure 3. Typical stone hammer used in the production of petroglyphs. Cerro Blanco, Mexico.

that is not the issue. The issue is one of falsifiability, which in all of archaeology simply does not apply—not without importing the methods of a hard science.

We have considered some variables affecting the methods of collecting the field data, using a small number of examples to illustrate falsities in acquisition strategies. But then this gathered material is also stored, interpreted and disseminated. At each of these stages there are subtle but systematic sources of contamination, error or bias. Storage and curation of Pleistocene osseous finds leads to several types of contamination affecting their susceptibility to analytical work. For instance mere excavation and storage destroys most DNA (Pruvost et al. 2007), while preservation treatment tends to eliminate dating analyses. The priorities in what materials to save and store and what to discard are of considerable consequences, and they are again subjected to cognitive biases, which may amplify those of the collector or they may be quite different. Which finds and materials are retained is to a large extent determined by the excavation's professed purpose, perhaps the orientation of those conducting or funding it, and most certainly by limitations of knowledge on their part. For instance the most important archaeological component of any sediment at the foot of a petroglyph panel are the stone hammers used in the production of the petroglyphs (Fig. 3), because their stratigraphical position is likely to tell us roughly at what time petroglyphs were made at the site. Thousands of such deposits have been excavated, usually in the hope of exposing rock art below ground in order to determine mere *minimum* dates for the art. This has succeeded in very few cases globally. But in the process of this mostly futile pursuit, the chance of securing *actual* ages of petroglyphs via stratified hammerstones was literally destroyed. An example are the over one hundred excavations in the Côa valley of Portugal, where not a single submerged petroglyph was found, yet the stone tools used in the production of the rock art were discarded because the archaeologists had not been trained in recognizing them (Swartz 1997; Bednarik 2004). This is a classical example of how inadequate knowledge destroyed the research potential of sites, and there are many other examples like this. For instance, nearly all the world's Lower Paleolithic finds of wood and resin, thousands of them, have been reported from just two countries, Germany and Israel. It would be foolhardy to suggest that such remains could only survive in these two regions; it is much more likely that archaeologists there are more adept in recognizing or securing fragile organic materials from excavations.

Biases in the eventual interpretation of the data are even more decisive, particularly as they are likely to change as the discipline's ideology vacillates historically. Pleistocene archaeology is entirely at the mercy of the historical sequence in which key discoveries are made—those that guide the dominant paradigms. In contrast to the systems of data gathering in most other disciplines, there can be little design in the acquisition strategies of Pleistocene archaeology. Most key finds are made fortuitously, yet they may decide how other aspects are interpreted. For instance when the period's first rock art was reported, from Altamira in Spain, it was completely rejected as we have seen in Lecture 1. Its sophistication was considered entirely incompatible with the perceived primitiveness of Upper Paleolithic people, as deduced from their earlier found tools. Yet it is obvious that if Paleolithic cave art had been discovered and reported first, it would have been the tools that would have been rejected as being contemporary, because *they* would have been regarded as incompatible with the sophistication of the art. In either case the perceptions and expectations of scholars will be significantly distorted, yet the order in which discoveries are made and accepted is largely accidental. Similarly, their acceptance depends on perfectly subjective factors at any time, and that certainly has been the case since the 19th century and *has continued right to the present*. Today many Pleistocene archaeologists reject the idea of symbolism and paleoart prior to 35,000 years ago, because they believe they already know what the cognitive levels of earlier humans were. Nothing has changed in the epistemology of Pleistocene archaeology, which suggests that we need to expect to see blunders as monumental as those of the past still occurring today.

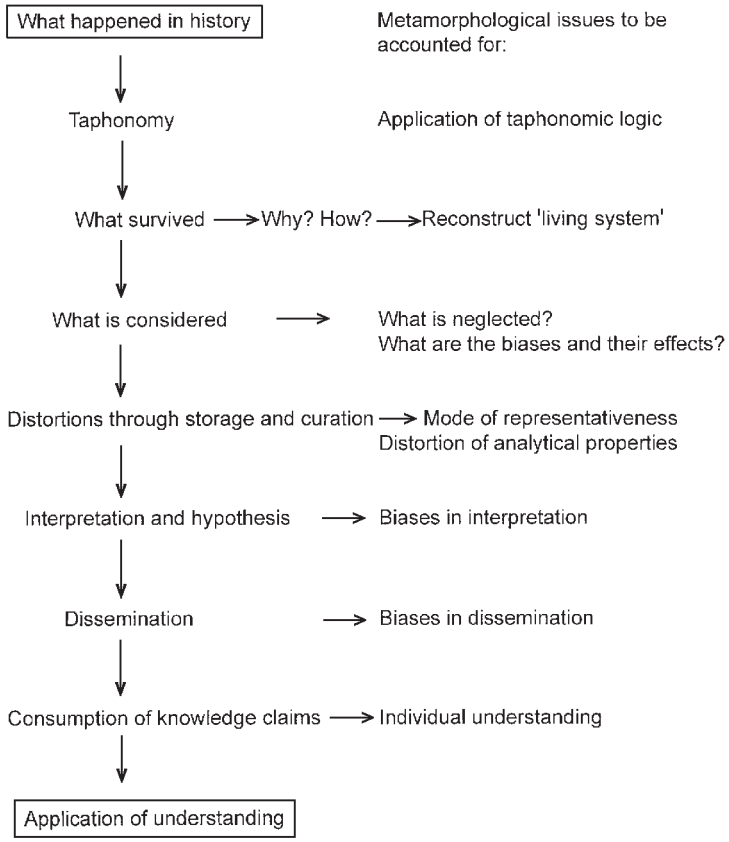

Figure 4. The principal factors in metamorphological analysis of archaeological knowledge claims.

The only thing systematic in this interdependence of sequence of discoveries, currency of paradigms and political currents of various types is that there will always be systematic distortions in the positing and acceptance of interpretations. These patterns can only change in kaleidoscopic modes as new evidence and new theories become available. Moreover, a powerful driving factor of archaeological theories about Pleistocene hominins is public perception. Rather than influencing it, the discipline curries favor with the public, on whose support it depends entirely, by constantly re-inventing itself according to social currents of society. The Neanderthal flower children of the 1960s became the victims of competition in the cynical 1990s, but today, with the failure of the market-driven economies and global climate change, their fortunes look much better again. In a few years their correct name, *Homo sapiens neanderthalensis*, will once again be reinstated by a discipline that changes its spots every time society modifies its views and priorities. One of the most disappointing aspects of archaeology is that it does not affect public perceptions; instead it merely adopts, amplifies and justifies them. It thus lacks authority and authenticity; it is simply a lackey of the state, ingratiating itself with the public to secure funding. The only certainty about archaeological interpretations is that they will always change, previous ones will be recycled, and new versions will be added from time to time. Clearly, this is not a scientific form of discourse and development. It is entirely dominated by non-scientific currents, and it is metamorphology's monumental task to unravel these intricate currents, to explain them, their interplay and their effects.

Biases of dissemination

The distortions resulting from the modes of collecting, storing and interpreting the materials Pleistocene archaeology is interested in are so great that the mission of metamorphology is indeed a huge endeavor (Fig. 4). In addition we still need to consider the distortions that inevitably occur in the *dissemination* of these data. The principal formal venues of dissemination are archaeological journals, which operate much in the same way as those of scientific disciplines. Papers about finds and interpretations are submitted, and are refereed by specialists in the particular field the author addresses. This system of peer review may work well in other fields, but in a non-scientific discipline it can become an agent of amplification of biases. The analyst then needs to understand the possible sources and principles of systematic processes. To begin with, an editor will select referees on the basis of her/his own experiences, and will be guided by several subjective factors. As in any field, intellectual cliques have formed in specific subject areas of Pleistocene archaeology, whose members may have developed narrow foci and exclusive views. If the paper complies with their collective model, they are likely to recommend its publication—often after requesting that their own work be cited in it. However, if the paper strays too far from the axioms of an influential paradigm, it is almost impossible to secure its publication, irrespective of its veracity or merits. The referees are likely to subscribe to a broadly based consensus model, and if there is no profoundly partial evidence in favor of the paper's contentions, they will be unwilling to forego the rewards that go with being the 'gatekeepers' of what is acceptable (a position they acquired through their being regarded 'right'). By the same token it is also true that the paper needs to present something new and different in order to be worthy of publication. Therefore an author's best strategy to get work into print is to endeavor offering some new ideas or data, but without upsetting the established order of the sub-discipline he addresses. The kind of paper finding the most ready acceptance presents new material that, in effect, confirms the existing dogma, and thus reinforces the positions of the 'gatekeepers'.

This role of disciplinary inertia is reinforced by other factors, among them the established hierarchy of scholarly serials. The world's archaeology journals are ranked according to perceived influence and prestige, which are essentially self-fulfilling perceptions. It matters greatly *where* the new work is published, but the greater the authority of a journal, the greater the reluctance of its editor and its referees to relinquish control of the dogma. They then have no choice but to reject a heretical paper that challenges the very foundations of the discipline's beliefs, and they are likely to reject even mildly iconoclastic work. While

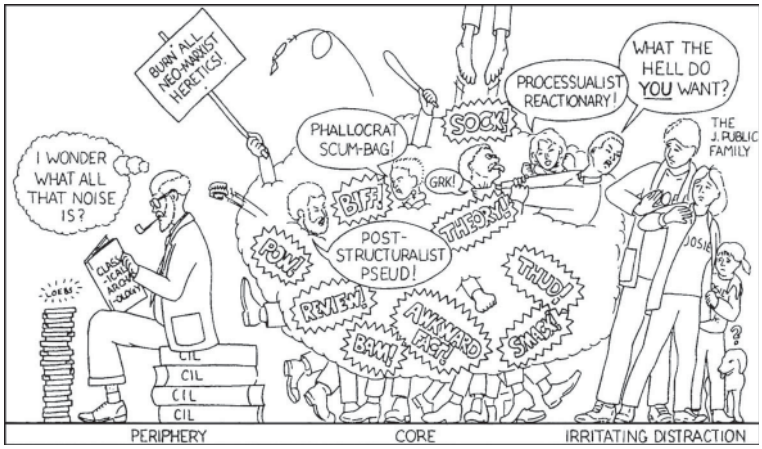

Figure 5. Animated archaeology (after Johnson 1999).

the hard sciences are required to submit to falsification, a non-falsifiable pursuit such as archaeology is not obliged to allow challenges. Therefore works of major heretical impact are extremely rare in this field, and should only be contemplated by strong individuals of immense dedication and intellectual resources (see Lecture 1).

Finally, in reviewing the dynamics of the dissemination of archaeological knowledge claims we arrive at the consumer of this information. If there is no collective subconscious to which all practitioners are somehow connected, and no uniform world archaeology or standard knowledge, as proposed here, it is inevitable that the reception of the conveyed information will differ significantly among recipients. The way it will be received, considered, processed and, especially, *applied* by the individual will vary greatly among archaeologists. These variations will be determined by individual knowledge, ability to reflect (e.g. upon criticisms), preoccupations or priorities, aspects of personal disposition (e.g. temperament), time constraints, and most especially by the prior conditioning of the practitioner concerned. All of these many factors need to be accounted for in a rigorous metamorphological understanding of how the individual receives and uses archaeological information to make sense within his or her reality construct.

None of this is intended as a criticism, it is mentioned here only in the context of the importance of these issues to the metamorphologist. S/he needs to understand the processes and pitfalls of dissemination: how there are systematic distortions to what has been found, analyzed, considered and written about, because of selective reporting. 'Unpopular finds' will not be reported in mainstream international journals, irrespective of how important they are (the Berekhat Ram proto-figurine is an example; Goren-Inbar 1986). Consequently very few scholars will even be aware of them, unless they end up entering the mainstream subsequently through some 'backdoor'. Favored paradigms will dominate the orthodox discourse, evidence supporting them will be over-emphasized, and evidence opposing them tends to be censured by the referee system. An example of the latter is the Professor Reiner Protsch affair in Germany (see Lecture 4), which was not hushed up because it discredited one academic, but because it significantly damaged the 'African Eve' hypothesis. It remains practically unknown outside of Germany. Thus these kinds of faddish theories may survive through practices of misinformation, and the hegemonic routines augmenting them need to be fully understood by the epistemologist of Pleistocene archaeology. As we have seen in Lecture 3, we have an endless variety of archaeologies around the world (Fig. 5), but by far the most prestigious and authoritative is the Anglo-American school. It alone determines archaeological dogmas these days, yet it is among the most 'inbred' traditions. In such a near-monopolistic environment, sectarian interests moderate the wider dissemination of knowledge and pluralist notions are discouraged. This renders the discipline quite susceptible to the amplification of mistakes, which are not only such a dominant factor in its past but remain so to the very present. There is no reason, from an epistemological perspective, why today's dominant models of Pleistocene archaeology could not be as spectacularly false as those we have visited in Lectures 1 and 4. The discipline's epistemology remains as defective as it was from the beginning.

The blowtorch of logic

Metamorphology can address this issue most effectively—which may well be why mainstream archaeologists are shunning it. Just like taphonomic logic, it tends to put the blowtorch of logic to the fantasies of archaeologists. I have edited archaeological works since 1964, in two languages, so I have seen thousands of papers that were not published because I had to reject them. So far this lecture has been an excursion into theory, and much of what has been said is rather abstract and perhaps not best suited to readily elucidate the issues raised here. A more effective way to communicate concerns with the average levels of archaeological understanding of archaeologists is to illustrate them by presenting a specific example and commenting on it analytically. I emphasize that the following example was selected entirely randomly; it is a passage from an academic paper I had to check only this week. This is not a particularly negative example (I could provide hundreds of similar or worse ones), it is from a paper by a university lecturer who holds a doctorate in archaeology. It describes a petroglyph site and this text is from a chapter entitled 'Analysis and interpretive reflections':

> Rock is immovable and permanent; for this reason men of all ages have used it to make graphic the ideas of their time, selecting flat surfaces of rock, often smoothing them to adapt the designs to the stone spaces available. Technical evidence indicates that the executors worked the designs directly on the granite rock, using soft percussion, pricking the segments of rock impacted; for rubbing, they might have used deer horn and hard wood. What is most certain is that the petroglyphs were created, leaving messages in their motifs of cupules and other visual figurations for us to study today. According to the methodology of ethnographic parallels, it is prescribed that the executors do not make what they see but rather

Figure 6. Section of the petroglyph sites described here.

what they know [see Fig. 6].

Rock is neither immovable nor permanent, in fact many quartz grains have undergone repeated phases as sand and sandstone, and the archaeologists who make a living removing rocks with petroglyphs would understandably disagree with this statement. The next part of the first sentence is an unsubstantiated platitude: we do not know the reason(s) why rock art was created, and seen in the context of taphonomic logic, the statement is nonsensical. Rock art would need to be seen as the only surviving form of an art tradition that may have included many other forms (body decoration, sand drawings, dendroglyphs; or art on clothing, tents, shields, other artifacts and so forth), which would render the statement false. Next we have the assumption that rocks were 'smoothed' before markings were made. This is an extremely rare practice and has not been observed in the region concerned.

Moving to the second sentence, we find the absurd assumption that petroglyphs on very hard rock (granite, apparently) were made by 'soft percussion' and 'pricking' (whatever these terms are supposed to mean), and by abrasion with antler and wood. This shows that the author lacks understanding of the relevant materials. The remaining two sentences are so naive a secondary school student could have written them. So we move on to the immediately following paragraph:

> In the upper stone, the arrangement of the cupules is circular, becoming more concentrated from outside to inside; that is, toward the interior of the carved space; the arrangement of the holes gives a sense of the parallelism imagined from ancient times in connection with the cosmic order and the existence of man, according to which intricate relations exist between the course of heavenly bodies and the thought of man. The changeless appearance of the engravings in the rocks would have converted the site into a sacred place. The proportions of relational symmetry achieved through the sculptural realism of a circular nature turn the group of cupules into an analogy representing the exterior space, where man would consider his reality to have been transmuted.

After a half sentence of description, the author abruptly delves into interpretation, which immediately descends into meaningless clichés. Here, the notion of 'interpretive reflections' becomes a euphemism for unfettered imagination and fantasy. Every single statement and phrase is offered without evidence, supporting reasoning, justification or deductive warrant. Indeed, the next paragraph consecrates the site without hesitation:

> The cupule is indifferent to the type of rock in terms of converting it into an altar of significant social value; the cult or ritual does not celebrate the rock itself; its significance arises from the encounter and where the ritual takes place. The second (lower) engraved stone displays a concentration of incised sculpted motifs with more variety, giving form to linear-geometric motifs that expand our information about the relation between the figurations and ideology. The three visible incisions form a grid in the space; its distribution in four parts would symbolize the spatial balance intuited for the cosmos, which the eyes of man distinguished in the succession of night to day.

At this point we need to be reminded that the site has not been described, we have little idea of what it comprises, of its morphology, topography or setting—or indeed any scientifically relevant information that might help us form an independent view of the site and its rock art; we are simply presented with a fait accompli identification as a ritual site of some cosmic properties. These interpretational inanities are presented as legitimate archaeological information, and they are justified in the next paragraph:

> Thus, we insist that actively analyzing the iconography is more important than simply describing the motifs. Among the constituent conditions of the acquisition of knowledge regarding the concrete realities of the world and universe is the perspective of our own view of the world, and the time in which we live.

These statements are most apposite in understanding the epistemology of archaeology. There is, first, the author's autosuggestive belief that the ideas presented amount to an 'analysis'. The word *analysis*, however, defines the separation of an entity into its constituent parts, whereas the various elaborations of the author seem to amount to a *synthesis* of subjective observations (vibes), personal beliefs and very vague ideological notions about people of the past (always bearing in mind that the author has no idea how old the rock art is, and therefore to which period or people it could possibly refer). The second sentence provides an interesting illumination of the author's professed perception of epistemology. He seems to espouse a laudable relativist 'view of the world', in which our perspectives are mere 'constituent conditions of the acquisition of knowledge'. But if he is guided by this understanding that we, the present people, do not adequately understand the world we exist in, how can he possibly take it upon himself to 'explain' the world or the motivations of past civilizations? Especially through a rock art he has no scientific grasp of, that he cannot attribute to any civilization, and that he cannot place into a framework of testing by taphonomic logic.

This brings us back to the fundamental shortcoming of archaeology. If we accept that as a species we are not in a cognitive state to fully comprehend our own reality, it would appear somewhat premature to blindly probe into the reali-

Figure 7. When African Eve's Moderns met the Robusts.

ties of others that lived long before us. Surely if we sought to explore the processes that led to the particular constructs of reality we subscribe to today, we would need to fathom their origins, which would presumably involve a rigorous exploration of the cognitive development of societies before ours. But how can we entrust such a delicate and demanding quest to a discipline that has so far only managed to generate a cacophony of mythologies about the distant past that are in reality often only reflections of *contingent beliefs and contemporary social constructs* (which we also understand inadequately)? It would be precipitate to base an inquiry into how hominins acquired their various constructs of reality on the models and views developed by such an undisciplined discipline as Pleistocene archaeology. It is here that metamorphology becomes absolutely indispensable.

Metamorphology, like taphonomic logic, not only cuts through the bombastic verbiage that marks so much of archaeology, it does this with the greatest ease and effectiveness. Consider the example of the claim that the locations of Paleolithic cave art prove that rituals took place in these caves, which we visited in the preceding Lecture. In analyzing this claim, metamorphology begins with observations of taphonomic logic: what is the 'crucial common denominator' of the phenomenon category (Bednarik 1990-91, 1994)? It finds that the CCD of 'cave art' is most probably not location, but almost certainly selective preservation. It then moves on to analyze how, beginning with a sophism, an entire mythology has been developed around it (e.g. that it is the oldest palaeoart). It then dismantles every part of this chain of probably false interpretations by showing that alternative explanations are either just as plausible, or, in most cases, even more so. Thus metamorphology transforms the dogmatic 'certainties' of Pleistocene archaeology into ambiguities, creating the systematic uncertainties science needs before it can consider scenarios of probabilities. It creates reliable knowledge by untangling the historically ossified 'received knowledge', separating it into its constituent parts, determining where these came from, how they were developed, who championed them, how reliable their basis really is, and other such aspects that, collectively, form the *epistemology of Pleistocene archaeology*.

And this is precisely where our task begins.

Return to Eve

At this point it is requisite to return to one of archaeology's most powerful dogmas of recent times, which we visited in Lecture 4: the 'African Eve' model (Fig. 7). The epistemologist would begin by asking: what are its constituent parts, where do they originate, how was this hypothesis developed and by whom? We have seen that this theory lacks supporting archaeological evidence: there is nothing to suggest that the Eurasian Upper Paleolithic technologies or palaeoart traditions were introduced from Africa. Rather, the evidence suggests that they were locally developed twenty to thirty millennia before the Middle Stone Age of northern Africa gave way to the Late Stone Age. I have suggested that the search for modern human origins is itself a misguided quest, because modernity is a function of culture and cognition, not of facial features or minor skeletal developments. Then there are the revelations of recent years that nearly all human remains of Europe the African Eve advocates had cited in the creation of their theories had been misdated, or their datings were in fact fake. Ultimately the model relies mainly on genetic differences among populations, and the principal support, after discarding all misconceptions and erroneous claims, derives from the notion that the distribution of modern genes reflects the mass-movements of people out of Africa. Minor support is also sought in the evidence that robust *Homo sapiens* people called 'Neanderthals' show slight differences in their DNA, relative to contemporary people.

Here we are not concerned with refuting this model, but only with how it came into being, how it established itself as dogma in much of the world, how it might illuminate the epistemology and promotion of dominant paradigms in general. It developed from a preceding, similar idea, expressed in the Afro-European *sapiens* hypothesis introduced in the early 1980s. That version (Bräuer 1984) was substantially based on the 'hoaxes' of Professor Protsch (Schulz 2004). By the late 1980s, British and American researchers had formulated the 'Eve' model, especially after a team at the University of California at Berkeley had subjected 136 mitochondrial DNA samples to a computer program designed by Alan Templeton, attempting to construct a family tree for 'modern humans'. They reported that we must all descend from one common mother that lived about 200,000 years ago. Dr Templeton then pointed out that the same data could have generated 10^{267} alternative and equally credible family trees (which is very much more than the number of elementary particles of the entire universe, about 10^{70}!), and the announcements were thus attributable to a computer bungle. Many other objections have been voiced, among them the apparent morphological continuities in European and especially Asian hominin populations, and Alan Mann's earlier finding that tooth enamel cellular traits showed a close link between 'Neanderthals' and present Europeans, which both differ from those of Africans (Weiss and Mann 1978).

Instead of abandoning their hypothesis, the Eve advocates tinkered with its details and continued promoting it so aggressively, through a few dominant journals, that it won rapid public approval, particularly in the English-speaking world. An important factor in the popularity of this model was the perception that it underpinned the idea of a single humanity, whose individuals are all ultimately related. But in fostering this feel-good notion of togetherness its academic backers overlooked two potential ideological objections: their tale of the rise of our ancestors who exterminated or out-competed all other humans on the planet involved a sinister side also (Bednarik and Kuckenburg 1999); and academic spin may foster academic careers, but it is detrimental to scientific veracity. At best, the claimed glorious triumph of our forebears would have come at a terrible cost to other humans; at worst it endorses fierce competition to the point of extinction and even becomes a rationalization of genocide. This ideology suited the sociopolitical climate of the 1990s, and that is perhaps why such a highly unlikely paradigm became adopted as virtual dogma. Its opponents found themselves just as marginalized and reduced as the supposedly replaced robust humans. Only a handful of them had the fortitude to profess the gradualist position through the 1990s.

But tribalism always comes at a price, the inevitable discrimination against an 'other'. The implicit argument is that the robusts were, after all, not around to object or suffer any ill effects, whereas the doctrine of universal kinship could bring today's nations closer. Academic partiality guided by political rationalization is common in archaeology, but it should not guide science. In the first few years of the current century, the tide began to turn against Eve, despite strenuous endeavors by her advocates to maintain the momentum of their crusade. Having probably realized that the cultural and technological evidence would yield no support for their cause, and that even their ascendancy in the paleoanthropological arena was under serious threat (particularly from the troublesome intermediate morphologies of so-called 'hybrids'), they focused increasingly on the new techniques of genetics. The thrust of the replacement scholars' tenet became to emphasize, as much as possible, the differences between hominins regarded as robust and those regarded as being Eve's progeny. Bones of 'Neanderthals' were analyzed, and minor differences in very fragmentary (and contaminated) DNA sequences were hailed as evidence that they must have been a different species. Moreover, the present distribution of DNA markers, it was claimed, indicates that today's humanity spread exclusively from Africa.

Again, we will ignore here why these claims are false (but see e.g. Vigilant et al. 1991; Barinaga 1992; Ayala 1996; Templeton 1996, 2002, 2005; Kidd et al. 1996; Brookfield 1997; Harpending et al. 1998; Pennisi 1999; Strauss 1999; Adcock et al. 2001; Fedele et al. 2002; Gutierrez et al. 2002; Hardy et al. 2005; Garrigan et al. 2005; Fedele and Giaccio 2007), and focus on how they might have come about. The notion of tribes wandering through uninhabited landscapes is particularly prominent, forming the demographic canvas facilitating the externalization of this gene fetishism. Colonizers of empty expanses of land are seen as the vessels of genes, and genes come to represent populations. Both assumptions are solipsisms: given enough time, genes can travel to the ends of the world without any mass movement of adequate numbers of people to replace resident populations (through generational mating site distances). And the idea of empty spaces permitting these Exodus-like migrations is as absurd as would be a belief that the Biblical Exodus did not result in the displacement of other tribes. Just as all ethnographically known hunter-forager-fisher peoples have occupied virtually all habitable regions of their world, the robust *Homo sapiens* people of Eurasia and Africa (and eventually Australia) had settled any part of these continents that was even remotely fit to live in, given their technologies. For instance the 'Neanderthals' have lived in the far north of Europe, even inside the Arctic Circle, where temperatures in the last Ice Age would have been well below today's -40ºC at times (Norrman 1997; Pavlov et al. 2001; Schulz 2002). This shows, firstly, that these supposedly primitive people must have been technologically for more advanced than most archaeologists give them credit for; and secondly, if they were prepared to accept such extremely harsh conditions, we can safely assume that the more liveable parts of the continent were all occupied before the mythical invaders (Bednarik 2008) from the tropics arrived. The residents were infinitely better adapted, both physically (having evolved in European conditions for several hundred millennia; Caldwell 2008) and technologically. Indeed, their culture and technology was clearly superior to that of northern African or Levantine populations between 40,000 and 30,000 years ago. Moreover, they were physically far more powerful (as we know from their skeletal muscle attachments) and had much more robust skulls and skeletons than the fragile Eve descendants are said to have introduced. The very idea that these well-adapted, technically superior, extensive robust populations that covered most of Europe would have allowed bands of unclothed tropical invaders to push into their bitterly cold territory and eventually wipe them out looks therefore rather preposterous. They would have simply swamped the newcomers with their genes, with their better adaptations, their superior cognition (as evidenced by their use of palaeoart), their resident status (any military strategist knows how hard it is to displace a resident population in armed conflict) and with their much greater numbers, physical strength and robusticity. The only sensible argument here is that of new diseases, but this works of course both ways.

Since we can safely assume that all suitable parts of the Old World were occupied during the Late Pleistocene, how did this false idea of largely unpopulated regions arise? It is clear from the perusal of the literature of the replacement advocates that they tend to see the distribution map of hominin finds from the period as somehow reflecting actual populations. For instance one writer sees Asia becoming populated by a 'chain' of 37 bands totaling 1110 people, expanding from Sinai to the Bay of Bengal, where they split into two 'chains', one of 61 bands and leading to Lantian in China, the other ending at Modjokerto in Java and comprising another 33 bands (Webb 2006: 20). This colonization

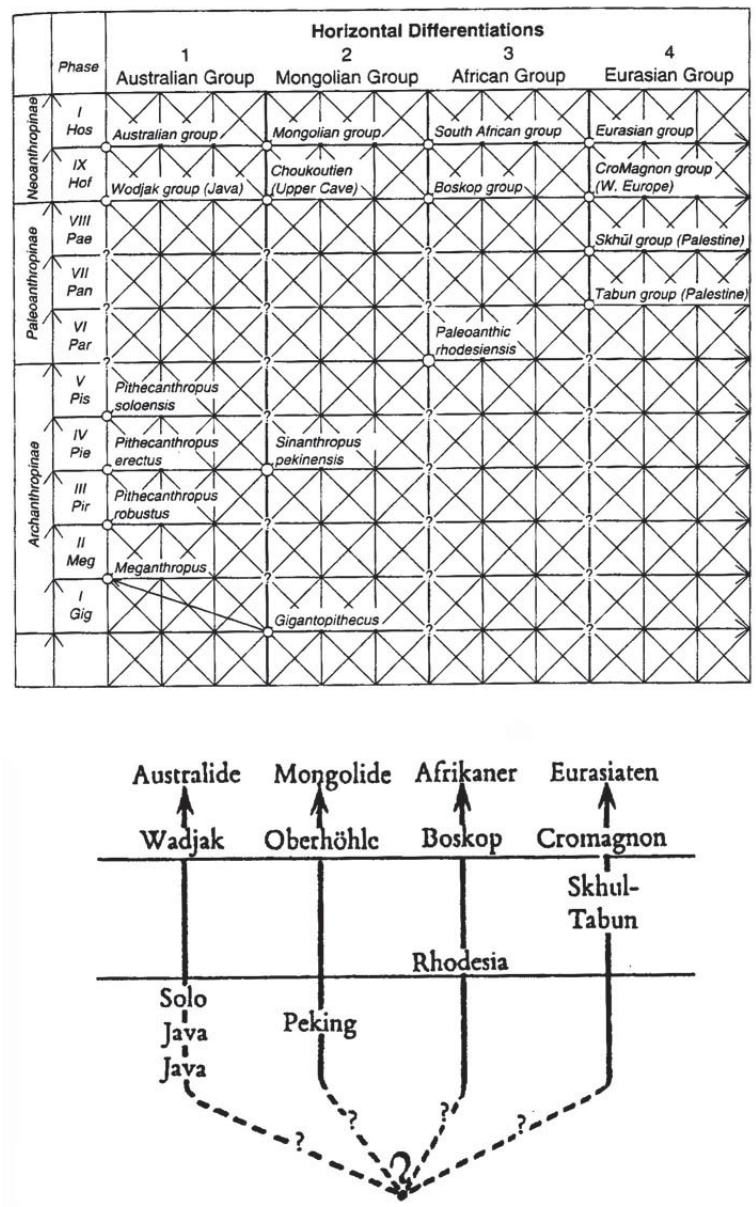

Figure 8. Above, Weidenreich's original trellis model of hominin evolution, which has been much misunderstood in Anglophone Pleistocene archaeology because the diagonal lines were not noticed. Below, Howells' false interpretation of Weidenreich's model.

scenario involved a total of 2820 people, and like all others has no credible basis of any kind.

Again there is an archaeological fetish involved: subliminally fossils represent people and populations. This illustrates once more how taphonomically illiterate most Pleistocene archaeologists are. Because they fail to comprehend the implications of taphonomic logic, they form entirely false constructs of the past. All variables relating to hominin remains, like those of all other animals, have undergone massive taphonomic distortions. On the whole, it is amazing that any have survived at all, and this is always attributable to fluke preservation conditions. Such materials only occur on very rare occasions, mostly in sheltered high-pH sediments that have not been subjected to such factors as frost action. Moreover, few of those that have survived have actually been recovered so far. To then derive from the map of their distribution (or of preserved occupation sites, for that matter) demographic deductions about former populations is illogical. What such a map *does* reflect is the distribution of where the best preservation conditions applied, *and* where researchers have so far looked. Again we see how expeditiously metamorphological examination detects fallacies in interpretation.

A sensible null-hypothesis would be to assume that, 45,000 years ago, all environments of four continents (plus at least twenty islands) permitting human colonization were as densely occupied by hominins as their carrying capacities permitted. Hence there were contiguous populations from southern Africa to Japan. These already possessed regional characteristics that had evolved over many hundreds of millennia in response to specific environments, diets and lifestyles. Reticulate introgression, genetic drift and episodic genetic isolation had occurred throughout, as Weidenreich's multiregional hypothesis had long maintained (Fig. 8). Most importantly, the movement of genes is much more plausibly explained by allele drift based on generational mating site distance rather than mass migration (Harpending et al. 1998). A mating site distance of merely 50 km per generation is most reasonable for such highly mobile populations, and it suffices to explain the travel of genes over 10,000 km in as few as 200 generations. Yet the enormous time scale available for the development of 'Moderns' amounts to perhaps 2000 generations. Thus the mosaic of human populations of the Final Pleistocene is the result of introgressive hybridization across contiguous populations subjected to minor demographic adjustments. The catastrophism of the Eve model, by contrast, demands firstly that a tiny population evolved in *complete* genetic isolation for hundreds of millennia in sub-Saharan Africa to the point where it could no longer breed with other humans. This is already quite absurd. Secondly it perceives some kind of bottleneck in which hominins became almost extinct in Africa. Genetic bottlenecks, however, tend to diminish fitness in the population (Bryant et al. 1986), rather than bring about the population's 'supremacy' (cf. Hawks et al. 2000), as the Eve model demands (consider endemic insular populations).

A second taphonomic flaw in the Eve model is that it assumes the archaeological and palaeoanthropological record we have of the Pleistocene is representative. Metamorphology shows this to be a fallacy. If the presumably more sedentary coastal populations in Europe had been more gracile than the more mobile tribes of the hinterland—the only ones we can have any evidence of—our perspective of Ice Age humans is necessarily so distorted it can only provide a parody of these societies. The notion of

invading Africans taking over Europe is as likely to be valid as the account of Noah's Ark: the more one thinks about the logistics, the more absurd it tends to become—except of course for the believers. Any rigorous epistemological review of this model reveals it to be an extremely unlikely demographic hypothesis, based on ideas of 'wandering' genes and on the geographical distribution of human fossils. Alternative explanations are much simpler and more parsimonious, and they are even supported by all the available evidence (Bednarik 2008).

At the conclusion of this lecture I return to an observation made above: we as a species are not in a cognitive state to fully comprehend our own reality. It would then appear somewhat hasty to probe without due care into the realities of others who lived long before us. I would like to illustrate this point by referring to the basis of all legitimate philosophy, so elegantly expressed in Plato's simile of the cave almost 2400 years ago (Neurath's "ship" defines much the same concept). Accordingly, humans lack access to Kantian objective reality ("*Das Ding an sich*"). Our species has created its many constructs of reality, which can only be assumed to be inadequate reflections of real realities. The reasons are of course complex, and the matter remains unresolved after millennia of philosophical endeavors by humanity's greatest minds. To illustrate the hopelessness of an unscientific, epistemologically unsound inquiry into the Pleistocene I would like to take the liberty of expanding Plato's simile:

> "Suppose there are these prisoners chained to the wall, and they see nothing but these shadows on a wall in front of them. That is all they know about the world. Except one more thing: they can also hear sounds made by some other, unseen prisoners in another chamber of the cave. They hear their groans and wailing, and they can recognize their humanness. But they cannot see them, they can only infer on the basis of their own understanding of reality. Which is so severely limited it is almost worthless in creating a valid construct of reality. What are the chances that the deductions of the first group, forming a picture of the second group, will be valid?"

We need to be a great deal more circumspect in extrapolating from our imperfect idea of the world to defining the world of people who existed eons ago; that much is certain.

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